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## HETEROGENESIS.

*Studies in Heterogenesis.* By H. Charlton Bastian, M.A., M.D., F.R.S. Pp. ix + 354 + xxxvii; 19 plates, with 815 illustrations from photomicrographs. (London: Williams and Norgate, 1903.) Price 31s. 6d.

HETEROGENESIS means, in these studies, the *per saltum* origin of forms of life from other quite different forms, e.g. of a ciliated infusorian from a rotifer's egg, or of a sun-animalcule from a chlorophyll corpuscle. It is long since Dr. H. Charlton Bastian first suggested this heresy; and many years of industrious observation have resulted in this large and expensive volume describing and (with 815 figures) illustrating those cases in which the author thinks he has detected the heterogenetic process at work. One cannot but admire the doggedness with which Dr. Bastian has persisted—*contra mundum*—in maintaining his thesis; and even those who feel quite sure that he has misinterpreted what he saw may find it interesting to discover by repetition of his experiments what did actually occur and was actually photographed. Others, again, who would not turn round to look at slides supposed to demonstrate that the egg of a rotifer may resolve itself into infusorians or into one large ciliate, may be more tolerant of the suggestion that Protistan evolution is still going on, retracing some of its ancient steps, or making new ones. It may be that Proteus still frisks a little among the Protists, or that there are mutations among unicellulars just as among De Vries's evening primroses.

It is often said by biologists that biogenesis (or the origin of approximately like from like) is not so much a law as a fact. It may not have always been, it may not everywhere be, that a living creature arises only from a parent or parents like itself; but, so far as our experience goes, the biologists say, there is no exception to this rule. Here, however, we have a book which is full of such exceptions, and yet the author's papers on this subject are rejected by the Royal Society of London, the Académie des Sciences of Paris, the K. Akademie der Wissenschaften of Berlin, and the R. Accademia dei Lincei of Rome! What does this mean? Does it mean more than that the author's rejected addresses appeared to the secretaries and committees of these learned bodies as incredible, as the recent facts and theories relating to radium would have seemed not long ago? "What can hardly be distinguished from perpetual motion, which it is an axiom of science to call impossible, has," says Prof. Boys, "left every chemist and physicist in a state of bewilderment." Thus, as the question rises in our minds, whether heterogenesis may not be an unknown factor in evolution, we turn to what Dr. Bastian has really done.

(1) The author believes that he has shown the *de novo* origin of bacteria within "closed cells," e.g. of fruits or in a structure like a caudal seta of Cyclops. In the last case, two or three days after death, "scarcely visible motionless specks gradually appear in the structureless protoplasm," they grow into bacteria and

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show active swarming movements. "In such a case it is clear that we have to do with no process of infection from without, but with a *de novo* origin of bacteria from the protoplasmic contents of the spines or setæ." But this interpretation is quite unnecessary; it has been shown beyond doubt that bacteria may exist in fluids where the highest powers of the microscope fail to detect them, many organisms normally swarm with bacteria, and the possibility of infection from without also remains.

(2) Another set of experiments, which appear to us of greater value, are concerned with the "proliferous pellicle" or zooglœa that forms on the surface of organic infusions. Apart from his conclusions, which means, of course, leaving Hamlet out of the play, Dr. Bastian's study of the surface pellicle, apparently a very hotbed of life, is a very interesting contribution to microbiology. From such a pellicle, which *seems* to consist solely of aggregates of bacteria, come fungus "germs," amoebæ, flagellate monads, and even ciliated infusorians like vorticellids. Some of the transformations are described and photographed from stage to stage, and a point is made of the diversity of results obtainable from similar pellicles. But, so far as we can see, no progress in proving heterogenesis can be made along this line. From what *seems* to be a homogeneous pellicle of discrete corpuscles there arise all sorts of animalcules, and Bastian argues in favour of heterogenesis because they were not there before; while to the majority of biologists the emergence of the amoebæ and infusorians simply proves that they *were*, in minute form, there before, or *were added* to the pellicle from above or from below in the course of the transformation.

It is a familiar fact that a small sample of water from a brook may show in many representative drops no evidence of living organisms of any kind, even when examined under very high magnification. But in this sample after a week or two there are not merely microbes, but monads, flagellates, and even ciliates. The enthusiast on behalf of heterogenesis concludes that the new tenants arose from ultra-microscopic animate particles or from non-living material; the ordinary humdrum biologist concludes that he overlooked in his sample the juvenile forms of the now obvious tenants, or that his expedients to avoid infection from without were insufficient. But, prejudice apart, it seems a little like a *non possumus* argument on both sides until we inquire into the familiar control experiments of boiling or otherwise sterilising the samples, and then the advocate of heterogenesis is forced to say that such cataclysmal operations as boiling prevented the inorganic potentialities from asserting themselves. This savours strongly of the explanation of a séance failure by the presence of sceptical spirits. It may be that in both cases the objection is valid, but when a full-fledged vorticellid emerges in the sample, or when the summoned spirit reveals an acquaintance with the canards of the daily Press, we fall back into utter scepticism in regard to both heterogenesis and spiritualistic séances.

(3) Many other heterogenetic modes of origin—of fungus "germs," of amoebæ, of monads, of Actino-

phrys, of Peranemata, and of diatoms—have been observed by Dr. Bastian. Thus he describes in detail how within the closed cells of sprigs of *Nitella opaca* multitudes of Actinophrys appear; “all the smallest specimens are of just the same size as the chlorophyll corpuscles,” “none are to be found smaller than these corpuscles,” they have at first no rays, and they are motionless. “The myriads of chlorophyll corpuscles are converted into the myriads of Actinophrys.” That is to say, the corpuscles of a vegetable cell may be suddenly transformed into well-known Protozoa with a specific cytoplasmic and nuclear structure, a *per saltum* transformation which no biologist would believe even if he seemed to see it, for it suggests a magical evolution entirely beyond credence, because so meaningless. In the same way the author describes the origin of immature diatoms as heterogenetic products due to the transformation of the cells of the alga *Chlorochytrium*, parasitic in the duckweed. Of course, the author discusses other interpretations, the infection hypothesis, backed by a further hypothesis of chemotaxis, and shows at length that they do not fit the facts he has observed. It may be noted that we have a very imperfect acquaintance with the complete life-histories of most unicellular organisms, that there is probably an extraordinary complexity of symbiosis and commensalism amongst them, that many are known to be very modifiable or plastic; and it may be that Dr. Bastian’s work will find its reward in provoking research to make our knowledge of unicellular organisms more continuous. On the other hand, it is well known that many forms carefully studied have a very definite and specific organisation, and we would believe many things—even that Dr. Bastian’s carefulness of method was not all that could be desired—rather than accept the view that one type of cytoplasmic organisation can be suddenly transformed into another. Evidence in favour of a certain amount of heterogenesis many biologists would be prepared to consider carefully, but when it comes to a chlorophyll corpuscle changing into an Actinophrys, we are in the position, absolutely inevitable, of those who would not believe in a resurrection though one rose from the dead.

(4) The climax of Dr. Bastian’s book is his account of the heterogenesis of ciliated infusorians. These relatively highly organised forms may arise, he says, from the pellicle on organic infusions, from the transformation of amoebæ, from encysted euglenæ, from the eggs of Tardigrada, and from the eggs of the rotifer, *Hydatina*. Let us confine ourselves to the last instance. It was found that the eggs of *Hydatina* may give rise in a few days to young forms of *Vorticella*, *Oxytricha*, *Aspidisca costata*, or to a large ciliate known as *Otostoma carteri*. Nine stages in the transformation of a fresh egg of *Hydatina* into an *Otostoma* are described, and the author says “however improbable this transformation may seem to those who have not studied the changes for themselves, the possibilities of error are still more improbable.” Against the suggestion that he mistook encysted *Otostoma* for the rotifer’s eggs, Bastian gives seven items of evidence; against the suggestion that each egg seeming to undergo the trans-

formation in question, was infected by an immature form of the infusorian, he gives eight items of evidence. If we take for granted that Dr. Bastian made no mistake in identifying either the *Hydatina* eggs or the large *Otostoma*, we must conclude that he witnessed a remarkable phenomenon which should be re-studied and properly figured. Different stages should be treated by the usual histological methods, carefully drawn and compared with the normal course of development. Careful attention should also be paid to the numerous parasites of rotifers, e.g. those described by Przesmycki. The photographic method used by Bastian has its obvious value, but the results are very far from clear or convincing.

In an exceedingly interesting and acute chapter on the general subject of discontinuous or *per saltum* variation, Dr. Bastian points out that his general position is supported by analogies in the inorganic world, e.g. by the fact that many substances crystallise in forms which belong to two or three different systems of crystallisation, and that the difference of crystalline form which they exhibit is associated with difference of specific gravity, hardness, colour, and other properties. Then there is the case of radium, which “may be looked upon as continuously giving rise to new elements by a process of material evolution.” He discusses cases of abrupt or transitory variation in animals and plants, and combats the opposition which Weismann expressed in his early “Studies in the Theory of Descent” to the idea of “sudden transformation of the whole organism.” But Weismann’s views have changed not a little since 1882, and it would have been fairer to have quoted from his “Vorträge” of 1902. Dr. Bastian does not seem to have quite realised how many biologists now accept, as proved up to the hilt, the frequent occurrence of Galton’s “transient,” or Bateson’s “discontinuous,” or De Vries’s “mutational” variations. But this is not quite the same thing as accepting the observational conclusion that the chlorophyll corpuscle of *Nitella* may become a sun-animalcule, or a rotifer’s egg a large ciliate! There are some things that one must see for oneself, and even then one would not believe them! It may be that “the lowest organisms exist at the present day because they are ever seething up anew by processes of heterogenesis”; and we should not be greatly surprised if that turned out to be a thesis “founded upon fact and consistent with reason.” But such an epoch-making conclusion must be proved, not by one prejudiced in its favour, but by a man of *thätige Skepsis*, who takes every possible precaution to avoid discovering heterogenesis, who will certainly not adduce as evidence cases of a rotifer’s germ-plasm demeaning itself to reincarnation as a ciliated infusorian. The author was forced, indeed, to offer the evidence that he observed, but if he had not drawn his bow quite so far as the case just cited implies, the arrow with “heterogenesis” on its feather might have penetrated further.

Very useful, it seems to us, is the author’s idea of “ephemeromorphs,” that is, of transitorily occurring phases among unicellolars, which may be wholly due to modificational influence, in contradistinction to the

regularly recurrent stable forms which constitute a species. We are inclined to think that this idea, extended to the study of multicellular forms, would show that many so-called species are based on transient " modificational " ephemeromorphs.

There is a dignified candour in Dr. Bastian's attitude towards sceptics. " It will doubtless be said that the facts I have brought forward are mere figments of my imagination, seeing that others observe no such phenomena, and that my experience is altogether exceptional." To this he answers (1) that many recorded phenomena, referred without proof to infection by parasites, are more simply interpretable as heterogenetic; (2) that he has seen the numerous cases he has recorded because he has diligently looked for them through many years; and (3) that, after all, his photographs of transformations have to be explained somehow. We must confess that the photographs, " engraved and printed," appear to us no more than a plethora of puzzles and futilities of delineation. The method is obviously admirable in being impersonal, but it seems to us quite ineffective in showing the transformation of the rotifer's or tardigrade's ova into infusorians. But we can only state our own impression; others may see more in them than we can detect; perhaps the eye of faith will see much.

" Some of my critics have refused to give any adequate consideration to the work because it has not been entirely done under certain impossible conditions which they would prescribe." Dr. Bastian admits that he has not uniformly isolated the organisms under process of change or placed them in a sterilised medium. Such cataclysmal interference would inevitably stop the heterogenetic progress. He declares, however, that the observations in proof of the heterogenetic origin of bacteria and their allies " have been conducted with all necessary precautions against the possibility of infection." If so, and every experimenter will agree that it is a big " if," then the bacteria did arise by heterogenesis, or they were present, though unseen, from the first. To the critic who asks why Dr. Bastian did not continuously watch the alleged heterogenetic changes from start to finish on the same individual organism, it is answered that the method pursued was that usually followed in embryological research—that of observing different states of change in different individuals. The method is comparable to that of palaeontologists in working out a presumed series of phyletic change; it is not absolutely demonstrative, but it reveals phenomena which must be interpreted somehow; and the best and most reasonable interpretation, " as much as possible irrespective of preconceptions and *a priori* views," seems to the author that of heterogenesis. If it be urged that Dr. Bastian should have watched the heterogenetic processes he believes in with the same sort of assiduous continuity as Dallinger and Drysdale achieved in studying their plastic monads, it is answered that " compliance with such demands would not only be fruitless but would go far to render for ever impossible any knowledge of heterogenesis." Why this should be so

we do not understand, unless heterogenesis be like the building of the fairies' palace which always stopped if anyone looked at it. But the fact is that " the methods employed by those who would gain a knowledge of heterogenesis cannot, from the very nature of the subject, be strict laboratory methods"—and this, we fear, will foreclose the question in the eyes of most biologists.

For our part, the suggestion of foreclosing a question like this savours of prejudice, and it should be remembered that results of great value, both theoretically and practically, have issued from the long drawn out controversy over spontaneous generation. After a careful study of Dr. Bastian's book, we venture to sum up our impressions as follows:—(1) If the idea of heterogenesis means, when generalised, that there may be marked discontinuity, or transience, or *per saltum* change in the ceaseless process of organic variation, then we side with the author, and in this regard he will find that he has many allies. (2) In so far as the idea of heterogenesis implies that many of the lowest forms of life are very plastic creatures of circumstance, capable of passing from phase to phase of the cell-cycle under modificational stimulus, so markedly that they may be called " ephemeromorphs," we again side with the author, for we think that there are many facts which point in this direction. (3) As to the thesis that simple organisms " are ever seething up anew by processes of heterogenesis," it seems to us, in our ignorance, a quite legitimate conception which may eventually be demonstrated as true. (4) But as to the majority of the cases of heterogenesis which Dr. Bastian adduces, we cannot but reject them as inconclusive, not only because the methods employed seem to us to be fallacious, especially in depreciating the possibilities of latent germs and of infection; not only because they lead us to conclusions which we cannot harmonise with our confessedly incomplete biological system; but especially because they are so *meaningless*. If the egg of the Hydatina can, " under conditions not always easy to realise," be transformed into a large ciliated infusorian, then our *Systema Naturae* is a farce.

J. A. T.

#### PROF. OSTWALD'S JUBILEE.

*Jubelband—Wilhelm Ostwald.* Gewidmet zur Feier seiner vor fünfundzwanzig Jahren erfolgter Doktorpromotion von seinen Schülern, mit einer Einleitung von J. H. van 't Hoff. Pp. xxxi+679. (*Zeitschrift für physikalische Chemie*, Band 46.)

**T**HIS stately volume is a fitting tribute to an eminent man who has done much to advance the progress of modern chemistry. The triumvirate, van 't Hoff, Arrhenius and Ostwald, the Dutchman, the Swede and the Russo-German, had a hard battle before their doctrines were accepted by physicists and by chemists. The communications made in the first volume of Ostwald's *Zeitschrift* by van 't Hoff (p. 481) on the rôle of osmotic pressure in the analogy between liquids